Center of Independent Experts (CIE) Independent Peer Review Report

Central Valley Winter Chinook Life Cycle Model (LCM)

David Hankin
Emeritus Professor of Fisheries Biology
Humboldt State University

Executive Summary

The winter-run Chinook Life Cycle Model (LCM) has very clearly been developed by an outstanding team of modelers using state-of-the-art Bayesian estimation and simulation methods. This team has developed a model which is tailored very specifically to the life history and dynamics of the winter-run race of Chinook in the Sacramento system, and it is clear that the team (and NMFS) has full "ownership" of the model, as recommended by Rose et al. (2011). The overall conceptual framework of the model (life stages, geographically important rearing areas for fry and juveniles, important transitions and associated survival probabilities, oceanographic impacts on early marine survival, ocean fishery impacts on immature subadults, etc.) seems fundamentally sound. The current LCM structure can be used to directly estimate transition or survival probabilities or, in some cases, to estimate values of parameters that are related to physical covariates (flow, water temperature, oceanographic conditions) that are linked to these transition or survival probabilities through simple functions that are often linear on the logit scale. Certain parameters can be assigned fixed values if they are well-identified independent of the LCM or if there is no apparent basis in collected data for their estimation, whereas values of other parameters can be estimated using Bayesian methods. It seems clear that the LCM in its existing state, through its linkage with several existing Central Valley water management models (e.g., CALSIM II, HEC-RAS, about which I profess to know very little!), can be used to compare the implications of alternative scenarios that affect physical parameters (flows, temperatures, habitat availability and access) that may affect life stage survival and abundance of winter-run Chinook salmon. Therefore, the model in its present form should be highly useful for comparing the *relative* impacts of alternative RPAs if the alternative RPAs can be expressed at a scale that matches or "fits" the scale and covariate structure of the LCM. In addition, the LCM incorporates a sophisticated "enhanced particle tracking model" that can apparently be used, by itself, to help guide day-to-day water management in the delta so as to reduce predation impacts (i.e., improve survival rates) through the delta. Overall, I judge the winter-run Chinook LCM to be a highly successful and impressive achievement, though with much work remaining to be done.

This reviewer confesses that he has never reviewed a model with the scope and complexity of the winter-run LCM, and he also confesses that he has not had direct experience with the sophisticated Bayesian estimation and population modeling approaches that have been adopted for use in the LCM. (Though I have certainly been exposed to these new Bayesian approaches for many years, I have not had occasion to become a serious "practitioner".) Within any large scale, highly complex and highly parameterized model, within which are imbedded many assumed relationships between environmental covariates (such as flows, temperatures, salinity, etc.), it is easy to find minor flaws or identify concerns (major or minor) at various specific areas within the model structure. From past experience, I know that identified areas of concern are typically closely linked to the areas of expertise of a reviewer! As an example, due to my background in dynamics and management of Chinook salmon fisheries, I know that the salmon analysis group at the NMFS Santa Cruz Ecology Lab has done outstanding analyses of ocean impacts and maturation schedules of winter-run Chinook from the Sacramento system, so my own concerns are not focused on that particular section of the LCM.

In my review, I identify and elaborate on a few specific issues with which I have greatest concern, and which I believe it is important for the model team to consider as they continue development of the winter-run LCM. These concerns are expressed as what I hope may be constructive improvements to the current LCM and include, in priority order: (1) development of a more plausible resident/migrant/survival model that moves fish from one habitat rearing area (specifically from the mainstem) to another rearing area (one of tidal rearing areas); (2) consideration of breaking the existing “mainstem” rearing area into two rearing areas: (a) Keswick Dam to Red Bluff Diversion Dam (RBDD, initial fry rearing), and (b) RBDD to Knights Landing/Yolo Bypass (additional rearing and movement from (a)), to improve biological realism of the resident/migrant/survival model, and to more closely align the model geographic structure to juvenile data that appear available from RBDD and from two screw traps deployed at Knights Landing; (3) improvement of the support (data-based and conceptual) for the calculations of habitat carrying capacity, with the key issues being (a) whether or not it is reasonable to use the 95th percentile of observed densities as the maximum possible density, and (b) whether it is reasonable that “marginal” habitat should contribute the majority of total habitat capacity; (4) explicit incorporation of effects of Livingston Stone Hatchery (LSH) on initial egg deposition of spawners (via removal of adults for hatchery spawning and initial rearing) and on initial fry/juvenile abundance (the team has indicated that it intends to do this); (5) reconsideration of current methods used to estimate egg deposition of spawners from existing estimates of escapement, with particular attention to likely substantial positive bias (towards females) in application of estimates of the proportion of females from Keswick Trap data; and (6) development of more transparent, rigorous, and objective *quantitative* criteria for evaluation of performance of the current winter-run LCM. I also believe that it is highly desirable for the LCM to somehow include process errors and the suggested used of stochastic binomial survival at all stages through model simulations, as opposed to deterministic survival, might be a suitable approach to this (assuming that I correctly understood the suggestion).

The extent to which the winter-run LCM approach can be extended to other runs of Chinook in the Sacramento-San Joaquin system cannot be definitively answered at present, because the winter-run model does not need to address key issues that impact dynamics of fall-run, spring-run and late-fall-run Chinook in the system. These key issues include at least: (a) existence of multiple run types of Chinook for which juveniles, likely of different mean sizes, will be “sharing” habitat capacity, thereby requiring development of inter-run competition relationships (not in the winter-run LCM); (b) existence of 5 large scale “production” hatcheries, all of which produce fall-run Chinook, and operation of which may have long-term consequences for fitness of natural-spawning Chinook from these other run types (LSH is a “conservation” hatchery and, as such, has been managed so as to minimize negative impacts on winter-run Chinook); (c) operations of these large scale production hatcheries may substantially change in the future (e.g., elimination of off-site release was recommended by the CA Hatchery Scientific Review Group and the nature of release locations has important implications for future fitness and genetic integrity of Chinook in the system); (d) it would seem critical to have a more detailed geographic representation of at least the mainstem rearing area (to accommodate the existing of populations in multiple tributaries and/or originating from the different hatcheries) and to add a San Joaquin geographic region, perhaps also subdivided to account for at least the hatcheries on the Mokelumne and Merced rivers; and (e) the resulting increased complexity and seemingly multi-run nature of an LCM for the other run types of Chinook salmon may make estimation, simulation and performance evaluation of such a model far more complex than for the winter-run population which is "comparatively simple".

Background

As a participant in the peer review process, I (1) read provided materials, and several other publications that appeared highly relevant, prior to a full day workshop, (2) attended the workshop and raised various issues with workshop presenters and other peer reviewers during the Santa Cruz workshop, and (3) discussed shared concerns and other matters with peer reviewers and session organizers during an approximately half-day session following the daylong workshop. After the Santa Cruz workshop, I (4) reviewed additional materials; (5) engaged in some quantitative analyses to help me better understand and express concerns with regard to two specific issues (the "fry survival/migration model", and assessing uncertainty in annual egg deposition) that are considered in further detail below; (6) contacted Noble Hendrix regarding (a) clarification of what model parameters were fixed as opposed to estimated, specifically in reference to the survival/migration model, and (b) data used to estimate sex ratio of spawners; and (7) reviewed the workshop summary report and some of the RPA documents distributed on 24 November 2015. I then prepared this document to constitute my written peer review. This review reflects my individual thoughts, though it has been influenced to a small degree by email exchanges with Jamie Gibson (one of the other reviewers) regarding some shared concerns with the current fry rearing/survival/migration model. Attachment 1 provides a listing of published and unpublished materials that I reviewed or accessed as part of my review activities prior to the most recent Google Share postings of 24 November (excluding some materials that were posted at the Google Share site).

Terms of Reference questions (TORs) were, for this reviewer, generally difficult to answer for two reasons: (1) certain terms in the TORs were not defined and were no doubt subject to alternative interpretations of meaning, and (2) reviewers were not provided with detailed information concerning Reasonable and Prudent Alternatives (RPAs) for flow management and other activities until long after the Santa Cruz workshop, too late to allow this reviewer to thoughtfully evaluate the degree to which the current LCM can allow assessment of the relative impact (survival through life stages and adult escapement) of alternative RPAs on the winter-run population of Chinook. Therefore, my responses to TORs have been fairly limited as compared to my suggestions for possible improvements in the LCM (summarized in the "Executive Summary" and "Additional Thoughts and Concerns" sections).

General Observations and Remarks

It is abundantly clear to me that (1) NMFS/SWFSC-FED has assembled a very highly qualified team of scientists to develop the winter-run Chinook life cycle model (LCM); (2) under Steve Lindley's leadership, the team has done an admirable job of responding to the Rose et al. (2011) edict to develop their "own" LCM tailored specifically to the winter-run population in the Sacramento River; (3) the scope and complexity of this model are greatly beyond the scope and complexity of any models with which I have directly worked myself; (4) the conceptual framework of the model seems (with some modest exceptions) fundamentally sound; and (5) the model, after some important modifications, should prove useful for contrasting relative impacts of alternative climate or water management scenarios (possibly including RPAs) on abundance of winter-run Chinook salmon. Given my own professional focus on very simple, almost "trivial", estimation problems involving only one or two related targets (e.g., estimation of a mean, proportion, total based on a design-based survey or using a mark-recapture estimator), or on analysis of fairly simple fishery dynamic models (e.g., single-species age-structured Ricker stock-recruitment models), I believe that rigorous assessment of the performance of a complex and very highly parameterized model like this must pose exceedingly complex and difficult issues. Therefore, my overall take on the winter-run Chinook LCM is that most of the appropriate pieces seem to be in place, that very good progress seems to have been made with parameter estimation, but that more work and thought needs to be given to development of rigorous criteria (ideally quantitative) for assessment of model performance (i.e., to objectively answer the question: "How well does this model really work?").

I also feel that it is important to note that, as a reviewer, I have been asked to review the merits of a model that is very clearly "in progress," and that reviewers had not been provided with a "formal document" that describes current model structure, estimation and performance attributes. Instead, we were provided with a 2014 "overview/sketch" of original model components, and with detailed PPT presentations that focused on evolution of the winter-run LFM to its current form. Therefore, as a reviewer, I have been asked to evaluate the merits and establish the attributes of a "moving target". For that reason, it is very likely that, at various places in the following review, my comments or thoughts may be off target due to a failure to understand current model structure or assumptions, or because one of my concerns may be lodged against an earlier version of the model which has already been modified so as to address that particular concern. Based on my reaction to the presentation by Noble Hendrix (on the underlying structure and the evolution of the LCM from Version "1" to Version "1.1"), all of the recent changes in model structure (introduction of egg to fry temperature covariate; inclusion of the enhanced particle tracking model for delta movements; separating spawning dynamics into monthly periods and accounting for adult sex ratios by age; and including ocean productivity covariates as factors influencing ocean survival rates) seem to generate important conceptual and predictive improvements to the LCM, and shifting to a new "adaptive MCMC" algorithm for parameter estimation seems to have greatly improved success of the parameter estimation process. Nevertheless, developers of the model concede that the current "observation error only" model structure needs to be modified so as to include random process variation (perhaps by simulating binomial variation in survivorship through all life stages), and that analysis within a full state space model structure would be ideal. Readers of this review should know that, although I have “kept up” in modern fisheries modeling sufficiently to appreciate the conceptual elegance of a full state space model structure, and I have reviewed and read journal articles based on application of this modern framework, I do not profess to be proficient with modern Bayesian methods for estimation of parameters of these highly complex models or simulations. My comments on estimation methods will therefore be limited.

Responses to Terms of Reference (TOR) Questions (exact language of TORs in red)

1) Is the model useful for informing NMFS of the effects of water operations and prescribed RPA actions on salmonids at various life stages and at the population level?

There are promising signs that the current version of the full winter-run Chinook LCM can be used to inform NMS of the *relative* impacts of water operations and prescribed RPA actions on winter-run Chinook at particular life stages and also at the “population level” (which I interpret as adult spawning escapement). If my understanding of the current model structure and methods of parameter estimation is correct, the LCM can be driven as a “simulation” model in the sense that if alternative sets of physical/hydraulic/temperature conditions are used as model inputs and previously estimated model parameters (based on past experienced conditions) associated with such covariates are used to drive the model over an identical time period, then one can compare simulated abundances at various life stages (including escapement) under alternative hypothetical scenarios. This kind of comparison is theoretically valid (assuming that the underlying model structure is reasonably sound and that estimated model parameters are reasonably well-identified or have a strong basis for their fixed values) and would allow assessment of whether or not a given RPA would or would not likely improve survival through stages, or boost actual escapement, relative to some alternative RPA. In the Noble Hendrix presentation, we were given explicit examples of how the model can generate statistics like “probability of higher spawner abundance (in year i) under scenario A vs scenario B”. If scenario B, associated with RPA B, were to increase spawning escapements in 93% of simulated years compared to those same years simulated under scenario A, then the LCM would imply that RPA B would have more positive population level impacts than RPA A.

a) What are the strengths and weaknesses of the model?

Strengths of the winter-run LCM include at least the following: (a) *well-identified life history stages*, (b) *well-identified model structure that seems to capture the essential steps* (egg to fry survival, fry survival and possible migration to tidal habitats, juvenile survival in freshwater or tidal habitats, migration to the ocean, survival in the ocean, ocean fishery impacts, maturation, return to spawn, and egg deposition) in the life cycle and effects of many physical and environmental factors believed to affect transition probabilities, (c) *state-of-the-art Bayesian estimation methods* for estimation of model parameters (including a matrix which apparently allows easy alteration of the set of parameters for which values are fixed and those which will be estimated), and a Bayesian population model framework that can apparently also be easily used instead for simulations of winter-run population response under alternative scenarios (e.g., alternative future climate regimes or certain alternative RPAs), (d) *incorporation of an intriguing "enhanced particle tracking model"* that seems especially useful for prediction of predation impacts through the Sacramento delta and which may provide useful guidance for day-to-day water management decisions in the delta, and (e*) very high quality information on fishery impacts, maturation schedule, and ocean survival* based on analyses of coded wire tag recovery data for winter-run Chinook released from Livingston Stone Hatchery (LSH) (see, e.g. Winship et al. 2014).

Overarching weaknesses of such a complex and highly parameterized model include at least the following: (a) valid objections can be made to many assumptions or assumed relationships that are used in the model, but it can in some cases be difficult or impossible to respond to these objections or, perhaps more importantly, to assess their importance within the larger model framework; (b) it may be very difficult to convey how the model works (model structure, assumptions and estimation procedures) to a lay audience (e.g., fishery and water managers) and key model assumptions are often left unstated; (c) it is extremely challenging to develop model performance metrics that allow rigorous and objective quantitative assessment of model performance (i.e., "how good is this model?" – more on this topic below), and (d) it is impossible to know if some alternative model structure might deliver similar or improved performance without development and analysis of that alternative model.

The above listing of what I perceive to be weaknesses of the winter-run LCM are not unique to this particular complex and highly parameterized model, but are weaknesses of any similarly complex and highly parameterized model. For that reason, in management of commercial fisheries, it is not uncommon to have "dueling assessment models" (see, e.g., "Improving Fish Stock Assessment Methods". 1998. NRC) with model performance measures agreed upon independently of model development.

In addition to the overarching weaknesses of the winter-run Chinook LCM, there are some specific areas, components or aspects of the overall model that appear relatively weakly supported or which I believe could and should be improved. In a review section titled "Additional Thoughts and Concerns" I attempt to provide constructive criticism which I hope may stimulate further improvement to the winter-run LCM or that may provide insight into construction of LCMs for other runs of Chinook salmon in the CV.

b) Are key parameters and performance measures captured in the model? If not, what other parameters and performance measures should be included?

I am guessing that the intention of this particular TOR question is to ask reviewers if they feel that most or all important relationships have been captured in the model, that there are reasonable specifications of covariates and functions associated with these relationships, and that “performance measures” here refers to simulated abundances of juvenile or adult life stages generated by the LCM. IF that is the intention of the question, then I can provide a generally positive response to this TOR question. One might object to the implicit rather than explicit treatment of juvenile growth in the current LCM, but I personally prefer the device of linking ocean survival rates to rearing areas (with areas believed to generate growth rates associated with higher ocean survival rates when smolts enter the ocean during years with poor survival conditions). I think it would be very tough to explicitly model growth of juvenile Chinook and I am not certain that the attempt would be worth the reward as the outcome (size-dependent ocean survival) would be the same as what the model now appears to deliver. On the other hand, as noted above, I believe that further work is needed to rigorously specify *model performance metrics* that will allow one to better judge how well the model seems to “fit” existing data, the degree to which model parameters have been well-identified, and the degree to which the model might one day be used for “predictive” (i.e., projection of future states) purposes as opposed to “comparative” purposes (relative performance of alternative RPAs) (see section titled “Development of Rigorous Performance Measures”).

c) Can the model be applied to address the multiple timescales associated with RPA decisions and operations?

This reviewer had no clear notion of the existing RPAs or of their history or explicit intent until the requested summary of RPAs was received on 24 November at which date I was headed off for a three-day Thanksgiving holiday with friends. As I skimmed through the various RPA documents that were sent out, including the letter detailing 2011 RPAs (from regional administrator, SW Region, NMFS to Mr. Donald Glaser, Regional Director, Mid-Pacific Region, U.S. BOR), my first impression was that the success or impact of most RPAs would be best judged not with a complex LCM, but instead with very specific on-site evaluation studies. My second impression is that I do not feel qualified to answer this TOR. During the Santa Cruz workshop, my impression had been that RPAs were a suite of identified actions that had not necessarily been taken, but that were felt likely to improve some aspects of habitat (e.g., access, temperatures, flows, gravel) and would improve survival and performance of anadromous salmonids in the Sacramento-San Joaquin system. As noted previously, the impact of alternative RPAs (e.g., expressed as alternative proposed flow regimes at various points) might be compared by running the winter-run LCM as a simulation model with alternative proposed flow regimes generating different physical covariates which should theoretically result in different survival through life stages and different population level (adult escapement) performance of winter-run Chinook under the alternative RPAs. But after skimming through the list of RPAs, my impression is that most (all?) RPAs are instead highly prescriptive, although some are “adaptive” (i.e., can be modified based on measured effects after an RPA has been implemented for some time), and that they are not expressed as alternatives but instead as established agreements concerning CV water operations (e.g., between NMFS and BOR to control flow at specific locations, support studies of green sturgeon, etc.).

As noted previously, I believe that the winter-run LCM, if run as a "simulation model", could be used to compare the relative impacts of *alternative scenarios* that generate alternative sets of covariates that affect survival and transition probabilities in the LCM. I do not believe that the relatively coarse geographic structure of the LCM would allow it to be used for evaluation of RPA impacts or comparison of alternative RPAs that had a very localized site-specific impact.

d) What are the technical constraints to the implementation of the model and the feasibility to address them (e.g., transparency of the model, data sets availability, model parameter uncertainties and sensitivities, etc.

Limited availability of data sets for winter-run Chinook, particularly for survival rates of small fry, does seem to pose constraints on model structure, which in turn must introduce uncertainty in estimation of model parameter values, etc., but I do not believe that this leads to "technical constraints to the implementation of the model" and I am uncertain what the "them" in "feasibility to address *them*" refers to. Based on Noble Hendrix's PPT at the workshop, and on other information obtained following the workshop, actual data on winter-run Chinook in the CV seem to consist primarily of the following: (1) estimates of adult spawning escapement from 1980-2015, with methods of estimation and associated uncertainty in estimates having three specific and quite different periods (and with associated lengths and sex of measured carcasses, 1998-present); (2) estimates of age-specific sex ratios among spawners based on fish collected at Keswick dam (of uncertain accuracy); (3) counts of juveniles believed to be winter-run Chinook (based on size) at Red Bluff Diversion Dam from 1995-1999 and 2002-2014; (4) collections of juvenile Chinook salmon made in rotary screw traps at Knights Landing, assumed to be winter-run Chinook based on size and timing (and associated relationships between counts and Sacramento River flows from del Rosario et al. 2013); (4) counts of fish collected in trawls at Chips Island and believed to be winter-run Chinook based on timing and size of fish; and (5) ocean CWT (coded wire tag) recovery data for hatchery-reared winter run Chinook released from Livingstone Stone Hatchery (used to calculate ocean fishery impacts, age-specific maturation rates). Although collections of Chinook at Knights Landing, not too far upstream from Sacramento, figure prominently in del Rosario et al.'s (2013) analyses of influence of flows on date that winter-run-sized fish pass the Knights Landing location, these data do not appear to be directly used as "observations" in the current version of the LCM (reasons for this are unclear). In the current model, the spatial resolution of the current LCM with respect to "mainstem" rearing seems too coarse to me. The current mainstem rearing area extends from Keswick Dam to the entrance to the Yolo Bypass ("floodplain"). Instead, it seems to me that the mainstem could/should be broken into two areas: (a) Keswick Dam to Red Bluff Diversion Dam (RBDD), and (b) RBDD to Knights Landing. With data collections made at both RBDD and Knights Landing, it would seem possible to improve model structure and ability to simulate survival and transition to tidal rearing with this additional specified region (more on this topic in the section titled "Additional Thoughts and Concerns").

2) Has NMFS effectively linked multiple specific models to represent the whole life cycle to inform NMFS in determining the effects of water operations and prescribed RPA actions on salmonids at the population level?

Based on my understanding of the LCM, the team has done an excellent job of linking existing CV water management models (CALSIM II, HEC-RAS) with the LCM and all potentially affected life stages (eggs, fry, rearing juveniles, migrating smolts, ocean-rearing sub-adults, maturing spawners) are explicitly identified in the LCM. In addition, the new enhanced particle tracking model has been linked to the LCM to improve simulation of survival (and predator impacts) through the delta.

3) Is the model framework suitable for winter-run, spring-run, and fall-run and can the framework be adapted for other species of Pacific salmonids?

With one very important caveat, I would have to answer this question affirmatively at a conceptual level. The overall general structure of the current LCM and approaches used for estimation of parameters, etc., could theoretically be extended to other races of Chinook salmon in the CV, though the developed model(s) would be considerably more complex than the winter-run LCM. The important caveat is that hatchery influence on the winter-run population has thus far (inexplicably, in my mind, see e.g., Winship et al. 2014 which concludes that the LSH program has increased the allowable harvest rate on the winter-run Chinook) been omitted from the current LCM, though reviewers were told that it is recognized as a required future component. Influence of a hatchery population on a naturally spawning population raises long-term genetic issues regarding fitness that are highly complex and remain highly controversial, and these long-term consequences are strongly influenced by hatchery practices. (See, e.g., recent reports of the CA Hatchery Scientific Review Group, 2012). Because fall-run Chinook in the Sacramento system are perhaps 90% or more composed of hatchery-origin fish and the winter-run Chinook LCM has not thus far addressed influence of hatchery fish, it is impossible for this reviewer to confidently state that the current winter-run LCM provides a reasonable template for building a LCM for the other Chinook salmon races in the CV.

There are additional reasons that development of an LCM for the other CV salmon runs would be a considerably more daunting (is that possible??!!) task than developing the existing LCM for winter-run Chinook. In addition to the absolute necessity to incorporate the substantial hatchery influence for fall run fish, (and also for spring run Chinook from Feather River hatchery), the need to consider multiple stocks from multiple river systems, and the much greater influence of ocean fishery removals on stock dynamics, competition for rearing space among wild and hatchery Chinook from these multiple stocks would surely be much more complicated than for winter-run Chinook. Thus, it would seem that a multi-race model (spring-run + fall-run + late fall-run) would be required. Development of an LCM for “steelhead” would also require incorporation of a substantial hatchery component and the various genetic issues that it raises, but would also need to address the very complex interactions between resident rainbow trout and anadromous steelhead (issues that have been addressed in work by

Satterthwaite, NMFS/SWFSC-FED).

4) Original: Can the model fit into a decision-making framework for using life cycle models (at appropriate temporal and spatial scales) to adapt water operations and prescribed RPA actions on individual and multiple species?

4) Revised: Is there evidence that the developed life cycle models can be placed within a relevant decision-making framework? (suggested revision proposed prior to the workshop) What are the key strengths? What is the model telling us more broadly? (added following the workshop)

During the workshop, we were provided direct (anecdotal) evidence that the new enhanced particle tracking model could be used for day-to-day decision-making involving routing of water through the delta so as to enhance survival (reducing predation impacts) of migrating juvenile winter-run Chinook. As we were not presented with any explicit decision-making framework within which the winter-run Chinook LCM might be embedded, it is impossible for this reviewer to provide an informed response to the original version of this TOR question. As noted previously, however, the existing model, even in its current form, appears useful for contrasting the relative impacts (on various life stages and on returning adults) of alternative water management scenarios, assuming that there are RPAs which can be expressed as alternative rather than prescribed actions.

Key strengths (and weaknesses) of the LCM are considered in TOR 1 (a). This reviewer is unable to understand the intent of the other question that was added to TOR 4 following the Santa Cruz workshop.

Comments on the Draft Summary Report (Criss and Beakes, 24 Nov 2015)

The Draft Summary Report seems to present an accurate summary of presentations and discussions at the Santa Cruz workshop. I do not believe, however, that the panel agreed to the revised language of TOR question 4 as presented in the Draft Summary. I believe that two sentences should be removed from revised TOR 4, because they are either previously addressed in other TORs (What are the key strengths?) or because they are too ambiguous to generate a meaningful reviewer response (What is the model telling us more broadly?).

Additional Thoughts and Concerns: Suggestions for Improvements to the Current LCM, and Development of Rigorous Performance Measures

Suggestions for Improvements to the Current LCM

Below I express my concerns regarding six key aspects of the current winter-run Chinook LCM in the hopes that expression of my concerns may lead to further improvements in the overall LCM or in presentation of the LCM to other interested parties. As noted previously, whenever a highly complex LCM is developed, there will always be many areas where model improvements are possible and it is impossible, I believe, to construct a complex LCM which cannot be improved upon, so I offer these expressions of concern in spirit of what I hope will be received as positive constructive criticism. Issues are considered in order of perceived importance and potential impact on model behavior.

1. Fry survival and migration from the area of spawning;

During the workshop, there was considerable discussion of the current model for survival and movement of fry from the initial spawning/fry rearing area to a tidal environment (Yolo Bypass, delta or SF Bay). This reviewer's impression was that all three reviewers shared misgivings about the current model and its behavior. The behavior of the current model is perhaps best illustrated by "pushing the model to its limits", an approach that I often take to check out whether or not a model is theoretically reasonable. If performance "at the limit" seems very seriously off target, then there are reasons to be concerned that something is wrong with model structure and there is a need to revise the model. I focus on behavior of the model as it moves surviving fish from the very long "mainstem" rearing area to floodplain or delta areas. From a geographic standpoint, this means moving surviving fish from "just below" Keswick Dam to "just below" Knights Landing, a distance of more than 131 miles (straight line distance).

The existing fry migration/survival model is (I think) as follows (note that time-specific notation was omitted in the 2014 LCM sketch).

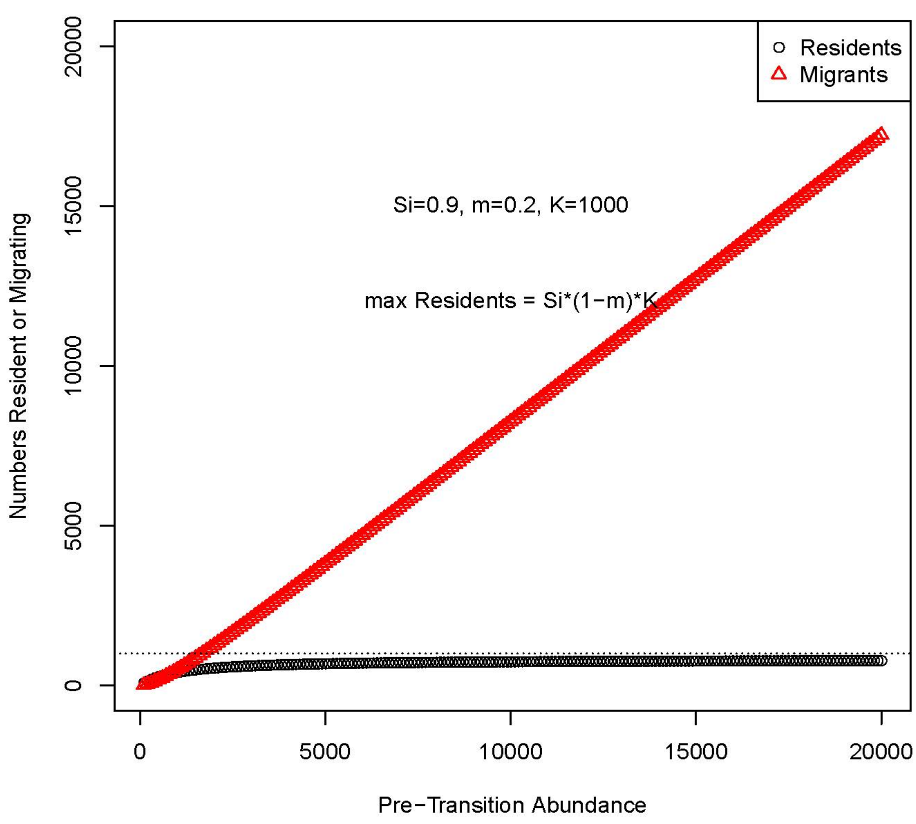
$$N_i(t+1) = \frac{S_i(1-m)N_i(t)}{(1+N_i(t)/K_i)}$$

where $N_i(t)$ is initial abundance of fry (i.e., "incoming fry" at the beginning of a one month time-step), S_i is a density-independent survival rate (though I assume that it may perhaps depend on environmental covariates such as temperature and flow), *m* is the proportion of fry that are "pre-programmed" to migrate in the absence of density-dependence (though *m* may also depend, I think, on Wilkins Slough flow), K_i is "carrying capacity" of habitat *i* , $N_i(t+1)$ is the number of fry remaining as resident after the end of a time step, and *i* denotes (current) habitat type, here equal to mainstem habitat. As the number of incoming fry becomes large, $N_i(t+1)$ approaches the asymptotic value of $S_i(1-m)N_i(t)$ (rather than K_i), but the more objectionable feature of this model is an assumption regarding the "migrants", $M_i(t+1)$, fish that move from the mainstem rearing area to tidal rearing areas during the period (*t, t+1*) (I think – again, time is omitted from notation in the 2014 sketch, leading to substantial ambiguity). Namely, the LCM currently appears to assume:

$$M_i(t+1) = S_i N_i(t) - N_i(t+1)$$

The figure below illustrates (dramatizes) the implications of this assumption as the number of incoming fry becomes very large (up to 20,000) compared to the carrying capacity (*K*=1,000), for $S_i = 0.9$ and m=0.2. Although the number of incoming fry that remain resident and survive to the next time period are indeed strongly restricted by the carrying capacity, the number of migrants that survival to a location more than 131 miles below the spawning rounds increases at an essentially linear rate once the number of incoming fry exceed about twice the carrying capacity. This model thus just "does not make biological sense". If the number of incoming fry were ever to be, say, 20X carrying capacity and fry had to move more than 131 miles from where they were born to where they might move (in the model) to tidal rearing habitats, surely many "excess" fry would die during their downstream movement to alternative rearing habitat, and survival rate for these "moving" fish would have to be density-dependent; many more fry would die than $(1-S_i)N_i(t)$. A model like the one presented at p. 15 of the 2014 LCM sketch would make sense, I think, only if the geographic area over which the model applied was very limited so that it would (perhaps) be reasonable to imagine that "essentially all" of those fish that did not remain as residents in the mainstem would survive to move out of the mainstem area to rear elsewhere.

Figure 7: S=.9, m=0.1, K=1000



Thus, it seems critical for the fry rearing/movement model to somehow incorporate not just an upper limit to the possible number of fish that could remain as rearing residents in the mainstem, but also to incorporate a density-dependent survival rate that affects survival of "moving" fry from the mainstem area to tidal areas. One very simple way of generated more reasonable behavior might be to instead assume

$$M_i(t+1) = f(N_i(t))[S_i N_i(t) - N_i(t+1)]$$

where $f(N_i(t))$ is some suitable function (e.g., logistic, starting at 1 at low density and declining to 0 (or a suitable low minimum) at very high densities) that reduces survival of "movers" as initial fry abundance increases well beyond the carrying capacity of the mainstem. (I am certain that there are a very large number of possible ways to construct a fry rearing/movement/mortality model that has more acceptable behavior, but this is not an area (juvenile rearing/migration) with which I am greatly familiar with existing models, so I am unable to suggest an off-the-shelf approach that could be used).

Additional Comments on this topic:

- I note that it is possible that I have greatly misinterpreted the behavior of the rearing/migration/survival models above due to a misunderstanding of time in the model (if so, I extend my apologies!). Absence of time-specific notation caused this reviewer substantial confusion. In particular, if, in the current version of the model, spawning of adults and hatching of fry are on a month-specific time step, would it not be critical to follow *cohorts* of fish through time and to link them? That is, during any interval (*t, t+1*), there are fry present that have reared for (in expectation) an average of 0.5 months previously, and these fry (or rearing fish) should be very much larger (and have greater survival) than those new incoming fry which the existing equations appear to describe. How does the model handle and differentially treat these successive groups of fry/juveniles as they survive and rear in the mainstem and/or survive and move to tidal areas for further rearing?
- Because abundances of winter-run fry have probably rarely exceeded carrying capacity over the period of simulation, it is unlikely that the objectionable features of the current rearing/survival/migration model have substantially affected estimation or simulation using the existing winter-run LCM. However, the objectionable model features could become critically important if the same rearing/survival/migration model were to be applied to fall-run Chinook for which hatchery releases may at times generate densities well above carrying capacities of habitats.

2. Data Availability and Regional Structure of Mainstem Rearing;

The above concern related to the existing fry survival/migration model seems to me to be closely related to shortcomings in the current geographic structure of the model which, at least superficially, could be addressed without adding substantial complexity to the current model. According to my understanding, existing data concerning winter run juveniles (as well as juveniles from other runs) originate from three main sources: (1) Red Bluff Diversion Dam (RBDD) measurements and counts of juveniles; (2) Knights Landing screw trap collections of juveniles (1998 – present, considered in Del Rosario et al. 2014), and (3) Chipps Island trawl collections of juveniles. Given these data sets, it makes good sense to structure the geography of the LCM model accordingly. That is, it would make good sense if the mainstem rearing habitat were divided into two distinct areas: Keswick Dam – RBDD (34.2 miles, straight line distance) and RBDD-Knights Landing/Yolo Bypass areas (97.1 miles, straight line distance). This geographic structure would seem to offer a better prospect for aligning/comparing model-simulated abundances with available data and might also assist in development and application of a more reasonable rearing/migration/survival model in the area of the river where initial abundance of winter-run fry must be greatest and where density-dependent reductions in survival rate would be expected to be greatest. It was unclear to this reviewer just why the mainstem could not have been represented by these two areas. Only one additional transition would appear needed (mainstem area 1 to mainstem area 2) and it would appear reasonable to assume that all surviving "residents" in mainstem area 1 must move to mainstem area 2 within "x" days that could presumably be based on historic winter-run juvenile data collected at RBDD and Knights Landing. Also, this revised geographic structure might motivate some alternative flow measurement within mainstem area 1 that might stimulate movement from area 1 to area 2. (In my review of del Rosario et al., I noted that the distance between Wilson Slough and Knights Landing is very small (less than 10 miles? I could not find exact distance and the paper merely stated "...at Wilkins Slough near Knights Landing…."). It makes no biological sense to me for flows essentially at the location of arrival to affect probability of movement from the equivalent of area 1 to the lower end of area 2. Instead, some flow further upstream in area 1 would logically motivate movement to area 2 and there would be a lag in response. That is, flow at Wilkins Slough is not itself a "trigger", but is correlated with some critical flow at some unknown upstream location/region.

I did not find the justification for the current designation of a single very long mainstem rearing habitat to be compelling, but I do not profess to fully understand the reasoning that generated this single large non-tidal rearing area. From discussions at the meeting it appears that rearing regions were selected to reflect political boundaries rather than existing data collection locations (see p. 11 of Draft Workshop Summary, Noble Hendrix speaking): "A difference in the CVC-LCM model is that it isn't structured to mirror where data are collected. Rather, the model is structured where political boundaries exists (e.g., where "Yolo" starts and stops)." This would explain the "Yolo Bypass" rearing area designation, but it does not explain the use of a single, extremely long "mainstem" region that would seem naturally separated according to data availability locations, without compromising the desire to create a "Yolo Bypass" rearing area.

Additional Comment on this Topic:

- Issues concerning geographic/regional structure of the LCM would seem to raise much more complex issues for development of an LCM for fall-run Chinook due to the tributary-specific "input" points of juveniles originating from hatcheries and natural spawning areas into the mainstem Sacramento River.

3. Habitat Capacity Calculations

I feel confident that the sophisticated mapping work that must have gone into development of the habitat capacity calculations was state-of-the-art and far more sophisticated than could be appreciated based on the 2014 LCM sketch, which we were provided prior to the Santa Cruz workshop and the PPT given during the workshop itself. We reviewed no formal document which might allow a reviewer to confidently critique methods used to generate the capacities for the specified habitats, but based on the materials reviewed, I wish to express the following concerns:

- Calculations appear to assume that habitat capacity (maximum sustainable fish/m^2) for a given category of habitat can be established from collected observations of densities of juveniles believed to be winter-run origin by using the 95th percentile of observed densities. For an endangered run of salmon for which current juvenile abundances must arguably be considerably less than those historically found in the Sacramento system, does this method of identifying capacity make sense? Further, could this approach be meaningfully extended to other runs for which hatchery influence is enormously greater than for the winter-run population and for which habitat capacities are likely exceeded when hatchery fish are released on-site (as had been general practice at the largest fall-run hatchery, Coleman NFH, until the current drought years)? Is there an alternative method for estimating habitat capacity (maximum sustainable density/area)? If instead maximum capacities are based on data collected in the Skagit River in WA, is that appropriate for winter Chinook in the Sacramento?

- Formulas used to move juveniles from one geographic area to another depend sensitively on specification of total habitat carrying capacity for "regional" levels habitats (mainstem, delta, etc.), emphasizing that habitat capacity calculations must critically influence model performance.

- On the final slide of the Greene et al. Habitat Capacity presentation, a listed uncertainty was that "most capacity comes from marginal habitats". My experience in fisheries research suggests to me that fish attempt to (and often exceed in) selecting "highest quality habitat" first and then move to lower quality habitat only after highest quality habitat has been saturated. Perhaps the LCM should conceptually consider use of, say, "high quality" and "low quality" habitat within each region, with associated different (implicit) growth rates and survival rates (explicit). Differential growth rates would then lead to differences in ocean survival in years when ocean conditions are poor (unfavorable for survival).

- At least one reviewer was concerned by the fact that the "preferred alternative" for flow regimes generated essentially no difference in habitat capacity as compared to the "no action alternative". Is this because the effects of water management are trivial compared to the interannual variation in mean flow (which is what really drives habitat capacity, especially in the Yolo Bypass, but also for mainstem "bank" habitat), or is it because capacity in "marginal habitat" masks important flow management-affected changes in "high quality habitat"?

4. Failure to Explicitly Incorporate Hatchery Influence

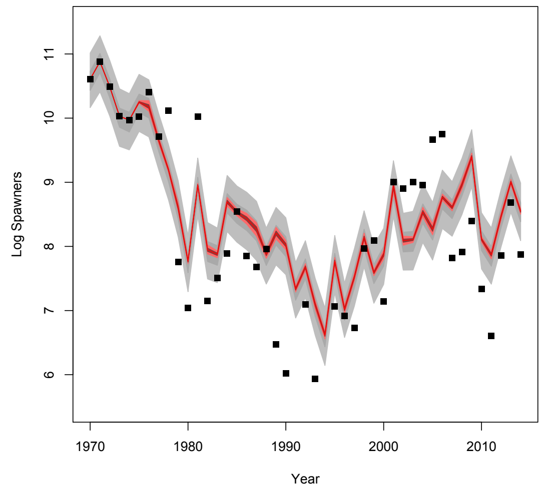
Given the possibly critical role that Livingston Stone Hatchery (LSH) may have been playing in dynamics of winter-run Chinook salmon, I was baffled by the absence of a hatchery component in the LCM. Because only unmarked adult winter-run are generally used in the LSH program and because marked (CWT) juveniles are released onto natural spawning areas at a relatively young age and without extended hatchery rearing, it would seem appropriate, as a first cut, to ignore long-term hatchery effects on fitness of the naturally-producing winter-run population. In that case, the dynamics of hatchery influence would appear relatively simple: (1) X(t) and Y(t) male and female adults are removed from the spawning escapement each year, and J(t) juveniles are added to the juveniles that are produced via hatching of naturally spawned eggs. Further, NMFS Santa Cruz has already assembled all relevant data and has carried out a peer-reviewed published analysis of the dynamics of the hatchery-assisted and (now) lightly exploited listed population (Winship et al. 2011, 2012). Incorporation of the hatchery influence should be a very obvious high priority and absolutely necessary modification of the current LCM.

Additional Comment on Hatchery Influence

Although incorporation of hatchery influence on the dynamics of winter-run Chinook may be relatively straightforward, it is by no means obvious that incorporation of hatchery influence on fall-run, spring-run and late-fall run will be straightforward. My understanding is that in most years, LSH operation has had a relatively modest influence on total abundance of winter-run Chinook and, in any event, LSH has been run as a "conservation hatchery" with intent to minimize possible long-term negative impacts on fitness of naturally-producing winter-run Chinook. The other salmon-producing hatcheries in the Central Valley are large scale production facilities. Salmon produced in these hatcheries have for quite some time dwarfed production from natural sources (an average of about 90% of fall-run adults in spawning areas are currently of hatchery origin – see CDFW reports on the relatively new constant fractional marking program), and hatchery release practices (primary large-scale use of off-site release of fish at locations (e.g., SF Bay) designed to circumvent survival issues in the delta and elsewhere) have apparently homogenized fall-run stocks (i.e., it is impossible to genetically distinguish fall-run adults from major Sacramento tributaries). Further, the recently completely California Hatchery Scientific Review has proposed many important changes in hatchery practices (including elimination of off-site release). Thus, an LCM for fall-run Chinook would need to account for the long-term impact of hatchery releases on dynamics and fitness of naturally spawning fish, and would likely require alternative simulation runs assuming different kinds of hatchery practices which would have different long-term implications for fitness of naturally-spawning fish.

5. Uncertainty in escapement estimates and initial egg deposition

In his presentation of the results of simulations using the current version of the winter-run LCM, Noble Hendrix noted that many of the observed log escapements were outside, often well outside, the 95% bounds (? – I am guessing) of repeated Bayesian simulation results (refer to reproduced graph below, with black squares indicating estimated adult escapements).



Normally, one would hope that most of the observed escapements would fall within the range of, say, 95% of the simulations (gray band (?)in figure above). It was suggested that some of the reason for these "overly confident", but off-target, simulation results is probably due to the fact that the current model incorporates measurement errors only, and excludes natural process variation. Another possibility that has occurred to me is that current simulations may not adequately reflect the uncertainty in annual egg deposition which is what "starts" the simulation of cohort dynamics and/or that adult escapements are not as well-identified as has been thought. Total egg deposition in year *t* can be expressed in at least two different fashions, depending on data availability. The first expression relies on mean age-specific fecundity, $\bar{F}_i$:

$$E(t) = \sum_{i=2}^{4} A_{if}(t)\bar{F}_i(t)$$

where *E* denotes total egg deposition, A_{if} denotes abundance of age *i* females, and the summation is over the ages of spawners (ages 2 ("jills") through 4). This formulation requires estimates of age-specific abundance and mean age-specific fecundity (which may vary across years). Error of estimation of total egg deposition will depend on error of estimation of total female escapement, error of estimation of the proportions of female escapement at age, and error of estimation of mean fecundity.

An alternative formulation of total egg deposition would be:

$$E(t) = \sum_{j=1}^{A_f(t)} F_j(t)$$

where $A_f(t)$ denotes total adult female escapement, *j* denotes an individual adult female in

$A_f(t)$, and $F_j(t)$ denotes fecundity (eggs) of individual *j* and assumed deposited on the spawning grounds. The alternative formulation is not of much practical value because the fecundities of individual fish are, of course, unknown, but Winship et al. (2011) provide a very detailed analysis of fecundity of winter-run Chinook based on fecundities of adult females spawned at LSH. They found that the best-fitting model relating individual fecundities to lengths of fish was the standard allometric model: $F_j(t) = aL_j(t)^b$, where L_j is fork length. On the linear scale used for fitting the model, $\log(F_j(t)) = a + b log(L_j(t))$, Winship et al. found that the best fitting model across years had constant slope (b), but varying a. Variation in the relationship between length and fecundity was quite modest across years, with the exception of a few years during which fecundities were unusually low (given fish length) compared to other years.

The relevance of the alternative expression for fecundity is that it suggests that the mean fecundity at age, used in the first expression, might vary across years as a function of the length frequencies of fish at age. Although the relationship between (expected) fecundity and length may not be highly variable across years, the length of fish at age may vary more substantially due to interannual variation in ocean conditions for growth.

The current version of the winter-run LCM does appear to use an age-structured representation for total egg deposition, allows distribution of spawning to vary across months, applies mean fecundities of 3,205 at age 2 and 5,000 at ages 3 and 4, and further assumes a maximum capacity for egg deposition due to spatial limitations in redd construction (slide 40 of Noble Hendrix presentation). The current model allows sex ratio of males and females to vary across years and, as an estimate of sex ratio among adults, uses Keswick trap counts of jacks, jills, adult males and females to estimate sex ratio at age 2 and at age 3 and 4 (slide 93).

My intuition suggests to me that uncertainty in annual egg deposition by winter-run Chinook may not have been adequately captured in the current LCM and would therefore contribute to the simulation performance pattern noted above (many adult escapements fall outside the typical range of simulation results). This reviewer does not have time to fully develop an approach to better account for uncertainty in egg deposition in the LCM simulations (ignoring the carrying capacity model invoked in the LCM), but I offer the following *very* preliminary analysis results that are summarized in the bullets below and the following table and figure:

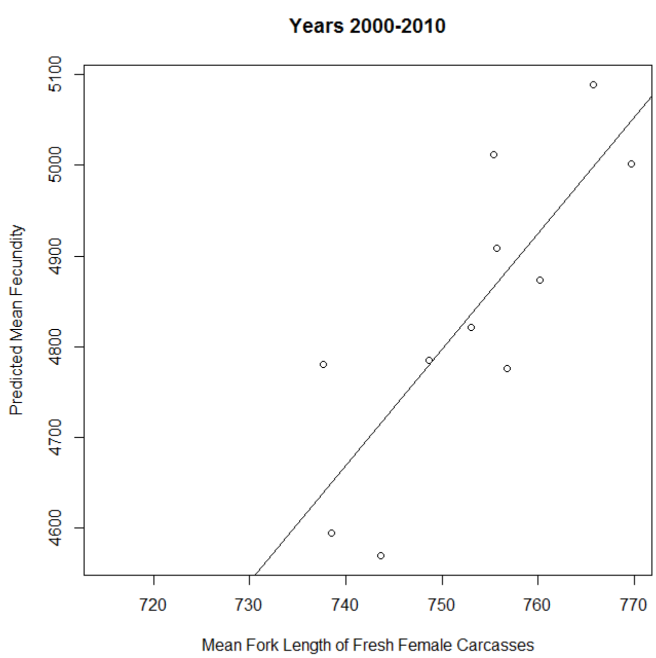
- Length and sex data collected from fresh female carcasses (and provided by USFWS) should generate a very good notion of the length frequencies of adult female spawners in a most years.
- Fecundity regression results presented in Winship et al. (2011) can be used to predict mean fecundity of these spawners by averaging the expected length-specific fecundities predicted from the Winship et al. regressions.
- For years beyond those examine by Winship et al., mean fecundity of female spawners can be estimated from a regression of predicted mean fecundities for individual years against mean fork length of female spawners (adjusted R^2=0.62).

- Carcass survey data are greatly dominated by females, presumably because spawning behaviors of females (protection of nest location until death) predispose them to be recovered on the spawning grounds whereas spawning behaviors of males (actively search for new female mates until near death, followed by passive movement downstream, away from the spawning grounds.

Calculations of mean fecundities of winter-run Chinook, 2000 – 2015, based on length frequencies of fresh female carcasses only (except sex ratio), and log-log fecundity vs fork length fits from Arliss et al. 2011 (exponentiated values, with no bias correction). Proportions of sampled carcasses that are female are indicated by Prop.Female.

Year	n.Fresh	Mean.Fresh.Fork	Var.Fresh.Fork	Mean.Pred.Fecund	Regress.Pred.Mean.Fecundity	Prop.Female
2000	1422	743.6392	3021.628	4569.456	NA	0.8298851
2001	1248	755.4062	3370.095	5011.451	NA	0.6544989
2002	1014	737.6696	3804.148	4780.831	NA	0.7584023
2003	2013	738.4749	2754.880	4594.329	NA	0.8325434
2004	1075	760.1191	3111.606	4873.470	NA	0.6905911
2005	3041	756.7747	2442.548	4776.433	NA	0.7458965
2006	1314	755.6613	2562.488	4908.339	NA	0.7065310
2007	557	769.6338	3564.977	5001.371	NA	0.7503153
2008	388	765.7474	2249.026	5089.440	NA	0.7392086
2009	562	753.0712	2055.524	4821.400	NA	0.7069168
2010	365	748.5945	4482.077	4785.031	NA	0.7423469
2011	163	731.7975	5417.212	NA	4563.584	0.7747748
2012	602	715.0050	1923.190	NA	4348.363	0.8206107
2013	1043	721.5724	1788.274	NA	4432.534	0.7859050
2014	560	749.1500	3765.062	NA	4785.982	0.6852699
2015	464	720.5797	2015.605	NA	4419.811	0.7172264

Prediction of Mean Fecundity from regression of Mean Fecundity Against Mean Fork Length



The above data and analyses suggest substantial, though not enormous, variation in mean fecundity of adult females. As relatively few adult female spawners are age 2, this suggests modest variation in mean fecundity of age 3 and 4 spawners.

Another and perhaps greater source of uncertainty in annual egg deposition by winter run females may be caused by uncertainty in the sex composition of spawners. Currently, the LCM apparently uses counts of male and female adults collected at Keswick Trap to estimate sex ratio and convert historic estimates of total adult spawning escapement to female spawning escapement. I believe that sex ratio estimates (more specifically, estimates of the proportion of spawners that are females) generated from these Keswick Trap data (kindly provided by Noble Hendrix) are perhaps quite seriously biased toward females (see earlier comments re spawning behaviors or females as compared to males):

Year	LgFemales	LgMales	Prop.Females
2003	137	64	0.6815920
2004	65	61	0.5158730
2005	225	144	0.6097561
2006	161	148	0.5210356
2007	103	55	0.6518987
2008	101	75	0.5738636
2009	166	101	0.6217228
2010	226	185	0.5498783
2011	193	87	0.6892857
2012	489	306	0.6150943
2013	178	100	0.6402878
2014	239	108	0.6887608

Given that estimated age 2 maturation rates (both sexes combined) typically ranged from 0.01 – 0.06 over brood years 1998-2005 (ignoring the anomalous 1999 brood with age 2 maturation = 0.16, see Table 3 on slide 36 of Noble Hendrix presentation), and given the very high estimated age 3 and age 4 maturation probabilities (quite similar for males and females), and even assuming that all age 2 returns are males, it seems highly unlikely that adult females would so dominate adult males on the spawning grounds. For example, assuming that age 2 maturation rate = 0.08 for males, assuming that only males mature at age 2, that age 3 and age 4 maturation rates are 0.90 and 1.00, respectively, for both males and females, that ocean survival rates are 0.5 between ages 2 and 3, and 0.8 between ages 3-4 are 0.8, and *assuming that sex ratio at ocean age 2 = 0.5* (note – this assumption could be wrong), the equilibrium proportion of females among age 3 and 4 adult winter run spawners would be (I think!):

$$\frac{A_{3f}^{*} + A_{4f}^{*}}{A_{3f}^{*} + A_{4f}^{*} + A_{3m}^{*} + A_{4m}^{*}}$$

=

$$\frac{[(1-0)*0.5*0.9*0.5R^{*}] + [(1-0)*0.5*(1-0.9)*0.8*0.5R^{*}]}{[(1-0)*0.5*0.9*0.5R^{*}] + [(1-0)*0.5*(1-0.9)*0.8*0.5R^{*}] + [(1-0.08)*0.5*0.9*0.5R^{*}] + [(1-0.08)*0.5*(1-0.9)*0.8*0.5R^{*}]}$$

= 0.521. In the above notation, R^* denotes equilibrium age 2 ocean recruitment, and A^* (with obvious subscripts) denotes age-specific equilibrium abundances of age 3 and 4 female and male spawners. If age 2 maturation rate for males were equal to 0.16, the equilibrium proportion of females would only be 0.543. If the age 2 maturation rate for males were equal to 0.32 (similar to the anomalous 1999 brood year age 2 maturation rate (both sexes combined) presented on slide 36), then equilibrium proportion of females would be 0.595. Available data provide little indication that age 2 maturation rate for males routinely reaches a value as high as 0.32, yet 8 of 12 female proportions estimated from the Keswick Trap suggest female proportions in excess of 0.595. Either these Keswick Trap data have strong positive bias toward females or the proportion of fish surviving to ocean age 2 is highly skewed toward females.

I am not familiar with the nitty-gritty details of just exactly how age- and size-specific fecundities, spawning escapement estimates and sex ratios together generate estimates of the total eggs carried by adult winter-run females in a given year, or across the entire sequence of years during which methods for estimation of escapement have changed, but I suggest that uncertainty in these calculations (spawning escapement of females and initial egg deposition of females) is probably greater than currently assumed. Perhaps the above thoughts could be reviewed by the NMFS Santa Cruz lab's salmon analysis group and their thoughts relayed to the team that has been developing and running the winter-run LCM.

6. Development of Rigorous Criteria for Evaluation of LCM Performance

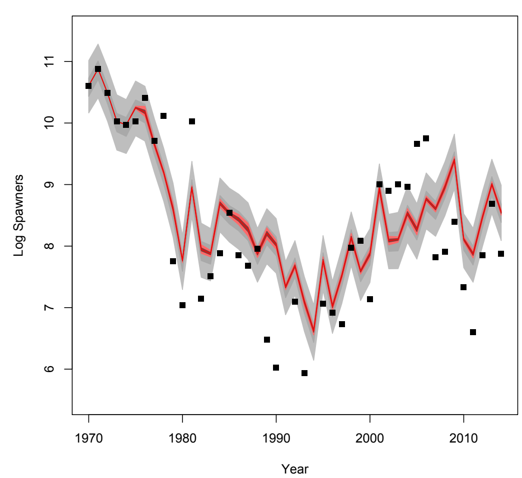
When I do my own "very small" work on relatively simple estimators in design-based sampling theory or mark-recapture settings, I have a very clear and statistically rigorous set of metrics with which to evaluate the performance of estimators: expected value (the average value that an estimator takes on over hypothetical repeated realizations of a sample survey or a mark-recapture experiment), bias (the difference between the expected value and the true target value of estimation), (sampling) variance (the average squared differences between sample- or experiment-specific estimates and the expected value of estimates, and mean square error (variance plus bias^2). When we think we have identified a "good estimator", we expect that it will be unbiased (or that bias will be very small compared to the target of estimation), that the square root of variance will be relatively small compared to the target of estimation (i.e., that the coefficient of variation of the estimator will be low, say 20% or less), and that mean square error is not much greater than variance (so that variance estimation will perform well). As I was reading materials prior to and after the workshop, and while I was listening to presentations given at the workshop, I kept wondering "What is/where is the clear set of objective criteria that will be used to evaluate the performance of this complex and highly parameterized LCM?" I believe that I saw glimpses of the answer to this question, but I did not feel that I was provided with a clear answer to the question that I had been pondering. I believe that more explicit attention needs to be given to development of rigorous, objective and, ideally, *quantitative*, criteria for characterization of model performance, especially because one could imagine constructing some other LCM (e.g. one based on energetic and behavioral considerations, with explicit modeling of growth rates in different habitats, with transitions from habitats motivated by growth rate (i.e., food limitation), etc.), and one might wish to compare performance of two competing LCMs (as in the dueling assessment example given previously).

Examples of possible model performance measures were indeed presented during the workshop, but not within a clearly "unified" format. Examples of *qualitative* model performance measures included at least the following:

- Qualitative comparison of prior probability distributions of parameter values with estimated posterior distributions. If the posterior distribution of an estimated parameter has essentially the same shape and location as the prior distribution, then we might conclude that that parameter was not well identified and we could somehow summarize that kind of comparison across all estimated parameters. (Note that we would surely also need some kind of rigorous justification for why certain model parameters were assigned fixed values whereas others are "fit to the data" using complex Bayesian methods. Indeed, there were suggestions that certain model parameters were (or should be?) fixed when estimated posterior distributions were judged "unacceptable" (see Noble Hendrix et al. slide 111 vs 112)).

- Qualitative examination of the posterior distribution itself. If this distribution is "too steep and too narrow", it may suggest that a parameter has been falsely identified as being estimated with high confidence.

- Qualitative comparison of the distribution of simulated model outcomes with available estimated attribute values (see, e.g., Noble Hendrix et al. slides 114, 115, 116, 117). Are there rigorous criteria with which one might evaluate model performance based on the kind of figure reproduced below?



Comments on the Review Process

Documents provided prior to the workshop were highly informative as were the generally excellent presentations that were given at the Santa Cruz workshop. Discussions with review panel members and presenters during the workshop were also highly informative as well as interesting. Online posting (using Google shared drive) of workshop presentations and reports was convenient, timely (with one notable exception) and effective. Instructions for preparation of reviews were extremely useful and seemed well thought through.

A more useful review may have emerged if a larger number of documents had been provided for review prior to the Workshop, especially if reviewers had been provided with a solid notion of RPAs, and if the length of the workshop process had been extended to a full two days. (I hope that my pre-workshop comment to try to keep pre-workshop reading to a reasonable level was not directly responsible for the very limited amount of pre-workshop materials!) Had this panel reviewer read through RPA documents (e.g., "Clean Version of the 2009 Reasonable and Prudent Alternative Revised to Include the 2011 Amendments") *prior* to the Santa Cruz workshop, a substantial portion of the discussions at that workshop would no doubt have been devoted to the possible connections between RPAs and the winter-run Chinook LCM. Instead, this reviewer has had to essentially abstain from arriving at any thoughtful conclusions concerning whether or not the winter-run LCM could be used for answering what seems to be one of the most important TOR questions: "Can the model be applied to address the multiple timescales associated with RPA decisions and operations?" This reviewer also struggled to interpret some of the TOR questions (in particular, original question (4) and certain language associated with revised question (4)) due to ambiguity in terms or usage. A good example is use of the term "performance measures" in TOR 1, which was undefined and therefore required reviewer interpretation of the term's meaning. This reviewer's interpretation of that term may possibly be at odds with the intent of the authors of the TOR question.

Addendum

In my original review, submitted on 01 December 2015, I stated that:

> Terms of Reference questions (TORs) were, for this reviewer, generally difficult to answer for two reasons: (1) certain terms in the TORs were not defined and were no doubt subject to alternative interpretations of meaning, and (2) reviewers were not provided with detailed information concerning Reasonable and Prudent Alternatives (RPAs) for flow management and other activities until long after the Santa Cruz workshop, too late to allow this reviewer to thoughtfully evaluate the degree to which the current LCM can allow assessment of the relative impact (survival through life stages and adult escapement) of alternative RPAs on the winter-run population of Chinook. Therefore, my responses to TORs have been fairly limited as compared to my suggestions for possible improvements in the LCM (summarized in the "Executive Summary" and "Additional Thoughts and Concerns" sections).

Following submission of my review, I devoted some hours to review of the late-arriving RPA materials (specifically, to review of the first 45 pages or so of the 2011 amendments to OCAP [040711_OCAP_opinion_2011_amendments_Enclosure 2_RPA] , and to review of the 07 April 2011 letter sent from Rod McGinnis (Regional Administrator, Southwest Region, NMFS) to Donald Glaser (Regional Director, Mid-Pacific Region, BOR), with the intention of elaborating on my review concerning those TORs that explicitly addressed the degree to which the current winter-run LCM can allow assessment of the relative impact of alternative RPAs. Having reviewed these materials, I offer the following thoughts.

- It is very tough to assess the degree to which the winter-run Chinook LCM can allow assessment of the merits or relative merits (in terms of increased rearing survival or spawning escapement of winter-run Chinook) of various RPAs, because the RPAs often have (a) multi-species objectives; (b) many RPA actions are targeted on a small local scale and it is unclear whether the water management/hydrology models can accurately incorporate relatively small changes in water management made at small local scales; and (c) the complexity of proposed RPAs, including locations and potential impacts of RPAs, and how these impacts may differ from "business as usual", make it difficult or impossible for someone outside Central Valley water management (i.e., a peer reviewer) to fully appreciate intentions of RPAs.

- For at least this reviewer, a thoughtful and useful response to TORs concerning RPAs would have required the Santa Cruz workshop to be of longer duration (at least 2 full days, but probably more like 2.5 days) with a least half a day devoted to discussion of RPAs, their history, their intention, and the degree to which RPAs are "flexible and adaptive" as opposed to rule-based and tightly prescribed actions.

- "Some" RPAs would clearly seem amenable to exploration of relative performance impacts (on winter-run survival rates and spawning escapement) via the current winter-run LCM IF they are expressed as alternatives or at least contrasted with "current" or "business as usual" actions. Because water management now seems based on the RPAs, however, I was often uncertain just what alternative water management regimes might involve (as all water management actions seem now obliged to be consistent with or directly follow the RPAs). Some illustrative examples of where the current winter-run LCM could be used to explore relative impacts of RPAs include at least the following:

- Action Suite I.2. Shasta Operations.

Proposed changes in Shasta Reservoir operations are designed to ensure, to the maximum extent possible, cold water during the period of spawning and egg incubation of winter run Chinook. Theoretically, existing water management models (like CALSIM) can accurately predict the effects of Shasta Reservoir operation, flows at Keswick, etc., on the temperature regime in upper Sacramento River where winter-run Chinook spawn. Temperature effects are directly included in the LCM via models of egg survivorship vs. temperature and recent modifications have distributed spawning over time in a more realistic fashion. It seems to me that simulation output from the current winter-run LCM could be used to fill out values in the unfilled Keswick release schedule table, at top of p. 21 of the 2011 revised OCAP.

- Action I.6.1. Restoration of Floodplain Rearing Habitat.

To the extent that these RPAs concern water management that affects access to Yolo Bypass, it would seem that the current winter-run LCM is designed so that it could produce simulations indicating whether or not these water management actions would indeed improve survival of winter-run Chinook via providing greater access to the Yolo Bypass rearing region.

- The multi-species objectives of many of the RPAs make this reviewer wonder if an assessment of the impacts of a particular RPA or set of RPAs on a specific species (e.g., winter-run Chinook) is, by itself, adequate to assess whether or not the RPAs have been wisely developed. For many RPAs, it would seem that positive impacts on a particular species (e.g., decreasing water temperatures via Keswick releases during winter-run spawning and egg incubation) might be associated with negative impacts on other species (e.g., higher Keswick releases during winter-run spawning/egg rearing might lead to reduced summer releases and reduced steelhead rearing survival, say). This issue cannot be addressed until LCMs have been developed for other affected species/races.

Documents reviewed

Prior to Meeting:

Rose, K., J. Anderson, M. McClure, G. Ruggerone. 2011. Salmonid integrated life cycle models workshop: Report of the Independent Workshop Panel. 28 pp. (provided by CIE).

Hendrix, N., A. Criss, E. Danner, C. M. Greene, H. Imaki, A. Pike, and S.T. Lindley. 2014. Life cycle modeling framework for Sacramento River winter-run Chinook salmon. NOAA-TM-NMFS-SWFSC-530. 27 pp. (provided by CIE).

Hood, W.G. 2006. A conceptual model of depositional, rather than erosional, tidal channel development in the rapidly prograding Skagit River delta (Washington, USA). Earth Surf. Proces. Landforms 31: 1824-1838. (secured by DGH).

Williams, J. 2013. Juvenile Chinook salmon (*Oncorhynchus tshawytscha*) in and around trhe San Francisco estuary. San Francisco Estuary and Watershed Science. 10(3): 1-24. (circulated by JW).

Winship, A.J., M.R. O'Farrell, M.S. Mohr. 2012. Management strategy evaluation applied to the conservation of an endangered population subject to incidental take. Biol. Cons. 158: 155-166. (secured by DGH).

Winship, A.J., M.R. O'Farrell, M.S. Mohr. 2014. Fishery and Hatchery Effects on an Endangered Salmon Population with Low Productivity. Trans. Am. Fish. Soc. 143:957–971. (secured by DGH).

Following meeting (secured by DGH):

Beechie, T.J., M. Liermann , E. M. Beamer & R. Henderson. 2005. A Classification of Habitat Types in a Large River and Their Use by Juvenile Salmonids. Trans. Am. Fish. Soc. 134:717–729.

Del Rosario, R.B., Y. Redler, K. Newman, P. Brandes, T. Sommer, K. Reece, R. Vincik. 2013. Migration patterns of juvenile winter-run-sized Chinook (*Oncorhynchus tshawytscha*) salmon through the Sacramento-San Joaquin delta. San Francisco Estuary and Watershed Science. 11(1): 1-21.

Winship, A.J., M.R. O'Farrell, M.S. Mohr. 2011. Estimation of parameters for the Sacramento River winter Chinook management strategy evaluation. Unpublished report. Fisheries Ecology Division, Southwest Fisheries Science Center, National Marine Fisheries Service, Santa Cruz, CA. 63p.

Data Accessed:
Length, Age and Sex data for fish collected in winter-run carcass surveys, 1998-2015. (From Kevin Orliss, USFWS).

Keswick Trap Sex Ratio Data (from Noble Hendrix).

Appendix 1: Review Material

Hendrix, N., Criss, A., Danner, E. Greene, C.M., Imaki, H., Pike, A., and S.T. Lindley, 2014. Life Cycle Modeling Framework for Sacramento River Winter-run Chinook Salmon, NOAA-TM-NMFS-SWFSC-530. (26 pages).

Rose, K., Anderson, J., McClure, M. and G. Ruggerone. 2011. Salmonid Integrated Life Cycle Models Workshop: Report of the Independent Workshop Panel. Organized by the Delta Science Panel. (28 pages).

Appendix 2: Statement of Work

External Independent Peer Review by the Center for Independent Experts

Central Valley Chinook Life Cycle Model Panel Review

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance the predetermined Terms of Reference (ToRs) of the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in **Annex 1**. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.org.

Project Description:

In April 2011, at the request of NMFS, the Delta Science Panel (DSP) convened an independent review panel to provide recommendations on how the agency should proceed with incorporating life cycle modeling of Chinook salmon into the ongoing analyses related to the Operations Criteria and Plan (OCAP), Biological Opinion (BiOp), and Reasonable Prudent Alternatives (RPA). The review panel reviewed existing models and considered four questions on model development. In June 2011, the review panel produced a report, *Salmonid Integrated Life Cycle Models Workshop: Report of the Independent Workshop Panel,* detailing their recommendations. One recommendation was that NMFS create a salmonid life cycle model tailored expressly for their purposes.

The Southwest Fisheries Science Center (SWFSC) has developed a new salmonid life cycle modeling framework which will be used to analyze water management scenarios on fish survival in the current development of the Biological Assessment (BA) for the Bay-Delta Conservation Plan. SWFSC is now requesting that a similar panel review the newly developed life cycle modeling framework. An independent panel review of the model will add credibility in its use in the BA scheduled to be completed in March 2016.

The Terms of Reference (ToRs) of the peer review are attached in **Annex 2**. The tentative agenda of the panel review meeting is attached in **Annex 3**.

Requirements for CIE Reviewers: Three CIE reviewers shall conduct an impartial and independent peer review in accordance with the SoW and ToRs herein. CIE reviewers should have expertise in water, habitat and fisheries management and coupled physical-biological models of freshwater or estuarine fish populations; landscape ecology; and knowledge of Pacific salmonid life history and ecology.

Each CIE reviewer's duties shall not exceed a maximum of 14 days to complete all work tasks of the peer review described herein.

Location of Peer Review: Each CIE reviewer shall conduct an independent peer review during the panel review meeting scheduled in **Santa Cruz, CA at the Southwest Fisheries Science Center's Fisheries Ecology Division** during November 5-6, 2015.

Statement of Tasks: Each CIE reviewers shall complete the following tasks in accordance with the SoW and Schedule of Milestones and Deliverables herein.

Prior to the Peer Review: Upon completion of the CIE reviewer selection by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later the date specified in the Schedule of Milestones and Deliverables. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, foreign national security clearance, and other information concerning pertinent meeting arrangements. The NMFS Project Contact is also responsible for providing the Chair a copy of the SoW in advance of the panel review meeting. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

Foreign National Security Clearance: When CIE reviewers participate during a panel review meeting at a government facility, the NMFS Project Contact is responsible for obtaining the Foreign National Security Clearance approval for CIE reviewers who are non-US citizens. For this reason, the CIE reviewers shall provide requested information (e.g., first and last name, contact information, gender, birth date, passport number, country of passport, travel dates, country of citizenship, country of current residence, and home country) to the NMFS Project Contact for the purpose of their security clearance, and this information shall be submitted at least 30 days before the peer review in accordance with the NOAA Deemed Export Technology Control Program NAO 207-12 regulations available at the Deemed Exports NAO website: http://deemedexports.noaa.gov/
http://deemedexports.noaa.gov/compliance_access_control_procedures/noaa-foreign-national-registration-system.html

Pre-review Background Documents: Two weeks before the peer review, the NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports for the peer review. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review.

Hendrix, N., Criss, A., Danner, E. Greene, C.M., Imaki, H., Pike, A., and S.T. Lindley, 2014. Life Cycle Modeling Framework for Sacramento River Winter-run Chinook Salmon, NOAA-TM-NMFS-SWFSC-530. (26 pages)

Rose, K., Anderson, J., McClure, M. and G. Ruggerone. 2011. Salmonid Integrated Life Cycle Models Workshop: Report of the Independent Workshop Panel. Organized by the Delta Science Panel. (28 pages)

Panel Review Meeting: Each CIE reviewer shall conduct the independent peer review in accordance with the SoW and ToRs, and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs cannot be made during the peer review, and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator.** Each CIE reviewer shall actively participate in a professional and respectful manner as a member of the meeting review panel, and their peer review tasks shall be focused on the ToRs as specified herein. The NMFS Project Contact is responsible for any facility arrangements (e.g., conference room for panel review meetings or teleconference arrangements). The NMFS Project Contact is responsible for ensuring that the Chair understands the contractual role of the CIE reviewers as specified herein. The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements, including the meeting facility arrangements.

The role of the panel is to review the framework for the Central Valley winter-run Chinook life cycle model developed by NOAA Fisheries SWFSC FED to determine whether NOAA Fisheries has fulfilled the recommendations given by Rose et al in the report, Salmonid Integrated Life Cycle Models Workshop: Report of the Independent Workshop Panel. The panel will appoint a chair and will use the Terms of Reference outlined in this document to guide their review. The chair will run the meeting and lead the development of a summary report on the second day of the review.

The specific responsibilities of the panel are to:
1. Review the technical documents listed above prior to the panel review.
2. Listen to presentations by project scientists describing the model framework.
3. Develop a summary report detailing whether NMFS has met the recommendations outlined in the report Salmonid Integrated Life Cycle Models Workshop: Report of the Independent Workshop Panel developed by Rose et al.

Contract Deliverables - Independent CIE Peer Review Reports: Each CIE reviewer shall complete an independent peer review report in accordance with the SoW. Each CIE reviewer shall complete the independent peer review according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2.

Other Tasks – Contribution to Summary Report: Each CIE reviewer may assist the Chair of the panel review meeting with contributions to the Summary Report, based on the terms of reference of the review. Each CIE reviewer is not required to reach a consensus, and should provide a brief summary of the reviewer's views on the summary of findings and conclusions reached by the review panel in accordance with the ToRs.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

1) Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review.
2) Participate during the panel review meeting in Santa Cruz, CA from 5-6 November 2015.
3) Conduct an independent peer review in accordance with the ToRs (**Annex 2**).
4) No later than 20 November 2015, each CIE reviewer shall submit an independent peer review report addressed to the "Center for Independent Experts," and sent to Dr. Manoj Shivlani, CIE Lead Coordinator, via email to *mshivlani@ntvifederal.com*, and Dr. David Die, the CIE Regional

Coordinator, via email to *ddie@rsmas.miami.edu*. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.

October 9, 2015	CIE sends reviewer contact information to the COTR, who then sends this to the NMFS Project Contact
October 22, 2015	NMFS Project Contact sends the CIE Reviewers the pre-review documents
November 5-6 2015	Each reviewer participates and conducts an independent peer review during the panel review meeting
November 20, 2015	CIE reviewers submit draft CIE independent peer review reports to the CIE Lead Coordinator and CIE Regional Coordinator
December 4, 2015	CIE submits CIE independent peer review reports to the COTR
December 8, 2015	The COTR distributes the final CIE reports to the NMFS Project Contact and regional Center Director

Modifications to the Statement of Work: This 'Time and Materials' task order may require an update or modification due to possible changes to the terms of reference or schedule of milestones resulting from the fishery management decision process of the NOAA Leadership, Fishery Management Council, and Council's SSC advisory committee. A request to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent changes. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on changes. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW and ToRs. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Applicable Performance Standards: The contract is successfully completed when the COTR provides final approval of the contract deliverables. The acceptance of the contract deliverables shall be based on three performance standards:
(1) The CIE report shall completed with the format and content in accordance with **Annex 1**,
(2) The CIE report shall address each ToR as specified in **Annex 2**,
(3) The CIE reports shall be delivered in a timely manner as specified in the schedule of milestones and

deliverables.

Distribution of Approved Deliverables: Upon acceptance by the COTR, the CIE Lead Coordinator shall send via e-mail the final CIE reports in *.PDF format to the COTR. The COTR will distribute the CIE reports to the NMFS Project Contact and Center Director.

Support Personnel:

Allen Shimada, COR
NMFS Office of Science and Technology
1315 East West Hwy, SSMC3, F/ST4, Silver Spring, MD 20910
Allen Shimada@noaa.gov Phone: 301-427-8174

Manoj Shivlani, CIE Lead Coordinator
Northern Taiga Ventures, Inc. Communications
10600 SW 131st Court, Miami, FL 33186
mshivlani@ntvifederal.com Phone: 305-968-7136

Key Personnel:

NMFS Project Contact:

Steve Lindley, Director
Fisheries Ecology Division
Southwest Fisheries Science Center
110 Shaffer Road
Santa Cruz, CA 95060
Steve.Lindley@noaa.gov
(831) 595-4653

Anne Criss, Assistant to the Director
Fisheries Ecology Division
Southwest Fisheries Science Center
110 Shaffer Road
Santa Cruz, CA 95060
Anne.Criss@noaa.gov
(831) 420-3996

Annex 1: Format and Contents of CIE Independent Peer Review Report

1. The CIE independent report shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations, and specify whether the science reviewed is the best scientific information available.

2. The main body of the reviewer report shall consist of a Background, Description of the Individual Reviewer's Role in the Review Activities, Summary of Findings for each ToR in which the weaknesses and strengths are described, and Conclusions and Recommendations in accordance with the ToRs.

   a. Reviewers should describe in their own words the review activities completed during the panel review meeting, including providing a brief summary of findings, of the science, conclusions, and recommendations.

   b. Reviewers should discuss their independent views on each ToR even if these were consistent with those of other panelists, and especially where there were divergent views.

   c. Reviewers should elaborate on any points raised in the Summary Report that they feel might require further clarification.

   d. Reviewers shall provide a critique of the NMFS review process, including suggestions for improvements of both process and products.

   e. The CIE independent report shall be a stand-alone document for others to understand the weaknesses and strengths of the science reviewed, regardless of whether or not they read the summary report. The CIE independent report shall be an independent peer review of each ToRs, and shall not simply repeat the contents of the summary report.

3. The reviewer report shall include the following appendices:

   Appendix 1: Bibliography of materials provided for review
   Appendix 2: A copy of the CIE Statement of Work
   Appendix 3: Panel Membership or other pertinent information from the panel review meeting.

Annex 2: Terms of Reference for the Peer Review

Central Valley Chinook Life Cycle Model Panel Review

1) Is the model useful for informing NMFS of the effects of water operations and prescribed RPA actions on salmonids at various life stages and at the population level?
    a) What are the strengths and weaknesses of the model?
    b) Are key parameters and performance measures captured in the model? If not, what other parameters and performance measures should be included?
    c) Can the model be applied to address the multiple timescales associated with RPA decisions and operations?
    d) What are the technical constraints to the implementation of the model and the feasibility to address them (e.g., transparency of the model, data sets availability, model parameter uncertainties and sensitivities, etc.)?

2) Has NMFS effectively linked multiple specific models to represent the whole life cycle to inform NMFS in determining the effects of water operations and prescribed RPA actions on salmonids at the population level?

3) Is the model framework suitable for winter-run, spring-run, and fall-run and can the framework be adapted for other species of Pacific salmonids?

4) Can the model fit into a decision-making framework for using life cycle models (at appropriate temporal and spatial scales) to adapt water operations and prescribed RPA actions on individual and multiple species?

Annex 3: Tentative Agenda

Central Valley Chinook Life Cycle Model Panel Review
Southwest Fisheries Science Center, 110 Shaffer Road, Santa Cruz, CA 95062
November 5-6, 2015, 8:30 am – 5:00 pm

First day	
8:30 am Arrival and coffee	
9:00 am Welcome and introductions	Steve Lindley
9:10 am Legal and Regulatory Context	Rea, McClain, or Yip (NMFS-CVO office)
9:30 am Project Overview	Steve Lindley
9:45 am Winter-run Life Cycle Model Framework Part 1	Noble Hendrix
10:45 am Break	
11:00 am Winter-run Life Cycle Model Framework Part 2	Noble Hendrix
12:00 pm Lunch	
1:15 pm Habitat Capacity	Correigh Greene
1:45 pm Enhanced Particle Tracking Model	Steve Lindley
2:15 pm Break	
2:30 pm Panel and Presenter Discussion	
4:30 pm Public Comment and Concluding Remarks	Steve Lindley
5:00 pm Adjourn	
Second Day	
9:00 Panel Report Preparation	

Point of contact for reviewer security & check-in

Anne Criss, Assistant to the Director
Fisheries Ecology Division
Southwest Fisheries Science Center
110 Shaffer Road
Santa Cruz, CA 95060
Anne.Criss@noaa.gov
(831) 420-3996

Appendix 3: Review Panel

Jamie Gibson, Contractor, Wolfville, Nova Scotia
David Hankin, Professor Emeritus, Department of Fisheries, College of Natural Resources and Sciences, Humboldt State University, California
John Williams, Consultant, Petrolia, California